EXHIBIT 2

UNITED STATES DISTRICT COURT
NORTHERN DISTRICT OF ILLINOIS
EASTERN DIVISION

ANDREW CORZO, SIA HENRY, ALEXANDER LEO-GUERRA, MICHAEL MAERLANDER, BRANDON PIYEVSKY, BENJAMIN SHUMATE, BRITTANY TATIANA WEAVER, and CAMERONE WILLIAMS, individually and on behalf of all others similarly situated,

Plaintiffs,

v.

BROWN UNIVERSITY, CALIFORNIA INSTITUTE OF TECHNOLOGY, UNIVERSITY OF CHICAGO, THE TRUSTEES OF COLUMBIA UNIVERSITY IN THE CITY OF NEW YORK, CORNELL UNIVERSITY, TRUSTEES OF DARTMOUTH COLLEGE, DUKE UNIVERSITY, EMORY UNIVERSITY, GEORGETOWN UNIVERSITY, THE JOHN HOPKINS UNIVERSITY, MASSACHUSETTS INSTITUTE OF TECHNOLOGY, NORTHWESTERN UNIVERSITY, UNIVERSITY OF NOTRE DAME DU LAC, THE TRUSTEES OF THE UNIVERSITY OF PENNSYLVANIA, WILLIAM MARSH RICE UNIVERSITY, VANDERBILT UNIVERSITY, and YALE UNIVERSITY,

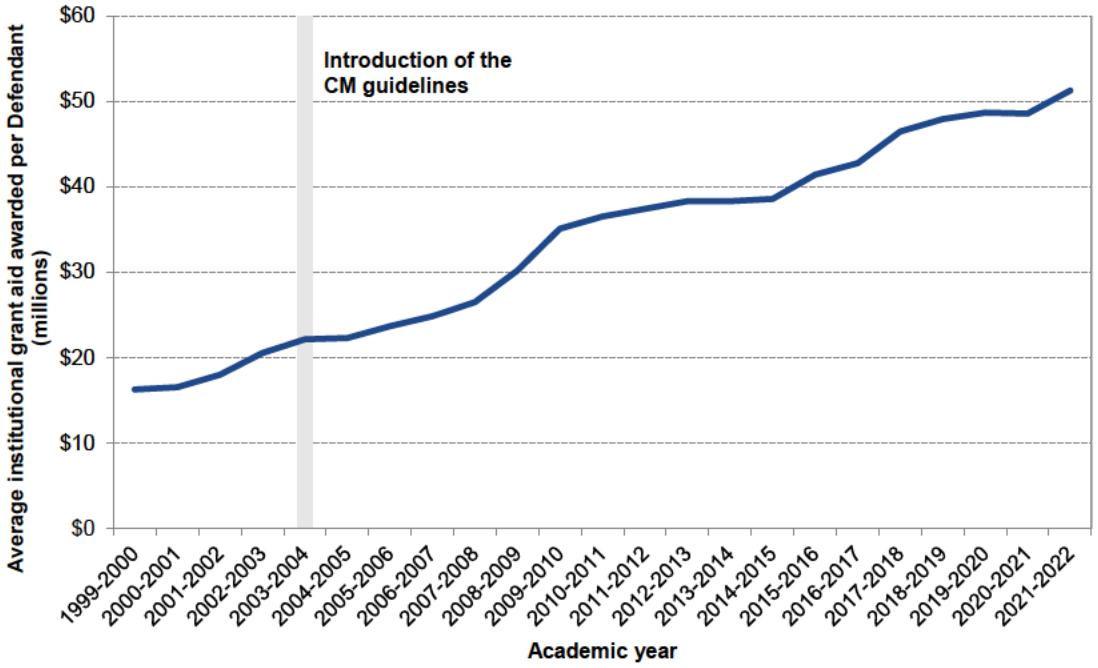
Defendants.

Case No. 22-cv-00125

EXPERT REPORT OF NICHOLAS HILL

7 August 2024

Figure 7: Average total institutional grant aid awarded per Defendant over time (dollars in 2024 prices)



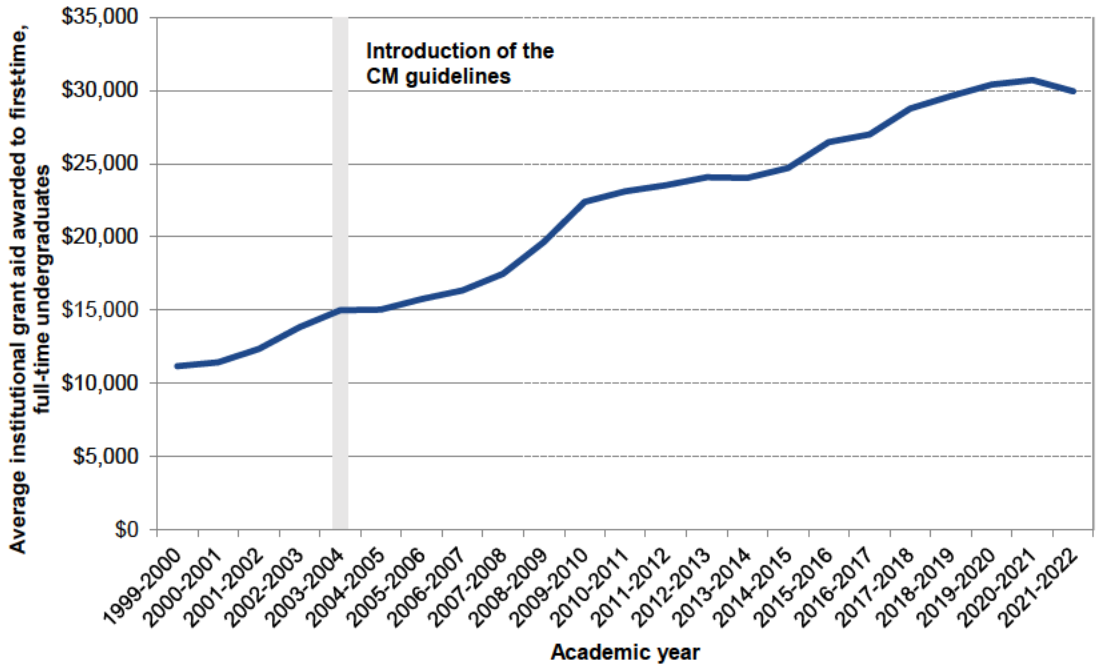
Sources: IPEDS; Federal Reserve Bank of St. Louis CPI data (as produced by Dr. Singer).
Notes:
[1] Institutional grant aid awarded per Defendant calculated as the average amount of institutional grant aid awarded to full-time, first-time degree-seeking undergraduates per Defendant (total institutional aid awarded to full-time, first-time, degree-seeking undergraduates / 17 Defendants).
[2] Aid awarded refers to grant or scholarship aid awarded to the student. Aid awarded may differ from aid dispersed to a student. For example, aid awarded may differ if a student leaves a school mid-year before the entire award amount is dispersed. "IPEDS 2023–2024 Student Financial aid survey instructions," Integrated Postsecondary Education Data System, accessed June 30, 2024, https://surveys.nces.ed.gov/ipeds/public/survey-materials/instructions?instructionid=30016.
[3] As previously discussed, the 2003–2004 academic year is the first academic year the consensus methodology guidelines could have potentially affected aid packages.

(81) Another way to quantify the amount of institutional aid awarded by a school is to calculate the average amount of institutional aid received per full-time, first-time, degree-seeking undergraduate student. I use the average institutional aid per student, as opposed to the average institutional aid per student who received aid, as a more complete measure for the total amount of institutional aid provided.[106] Figure 8 summarizes the average amount of aid received per full-time, first-time, degree-

[106] To see this, consider two otherwise identical schools A and B with a cost of attendance of $10,000. School A provides institutional aid for 50 percent of the cost of attendance for 50 percent of students. School B provides institutional aid for 50 percent of the cost of attendance for 50 percent of students and institutional aid for 10 percent of the cost of attendance for the rest of 50 percent of students. In this case, School B provides more financial aid to students overall. The average institutional aid per student is calculated at $2,500 for School A and $3,000 for School B, whereas the average institutional aid per student who received an award is calculated at $5,000 for School A and $3,000 for School B, which is inconsistent with the fact that School B provides more institutional aid. Such a situation may occur for a school like MIT, which "provided a $5,000 Covid-era grant to all enrolled undergraduate students" during the pandemic. Office of the Vice Chancellor, "MIT keeps a firm commitment to undergraduate student support for 2021–2022," MIT News, March 26, 2021, https://news.mit.edu/2021/mit-keeps-firm-commitment-undergraduate-student-support-0326.

seeking undergraduate student by year at the Defendants. The figure shows that inflation-adjusted average institutional aid awarded by the Defendants per undergraduate student has risen both over time and since the introduction of the consensus methodology guidelines.[107]

Figure 8: Average institutional grant aid awarded to full-time, first-time, degree-seeking undergraduates by the Defendants (dollars in 2024 prices)



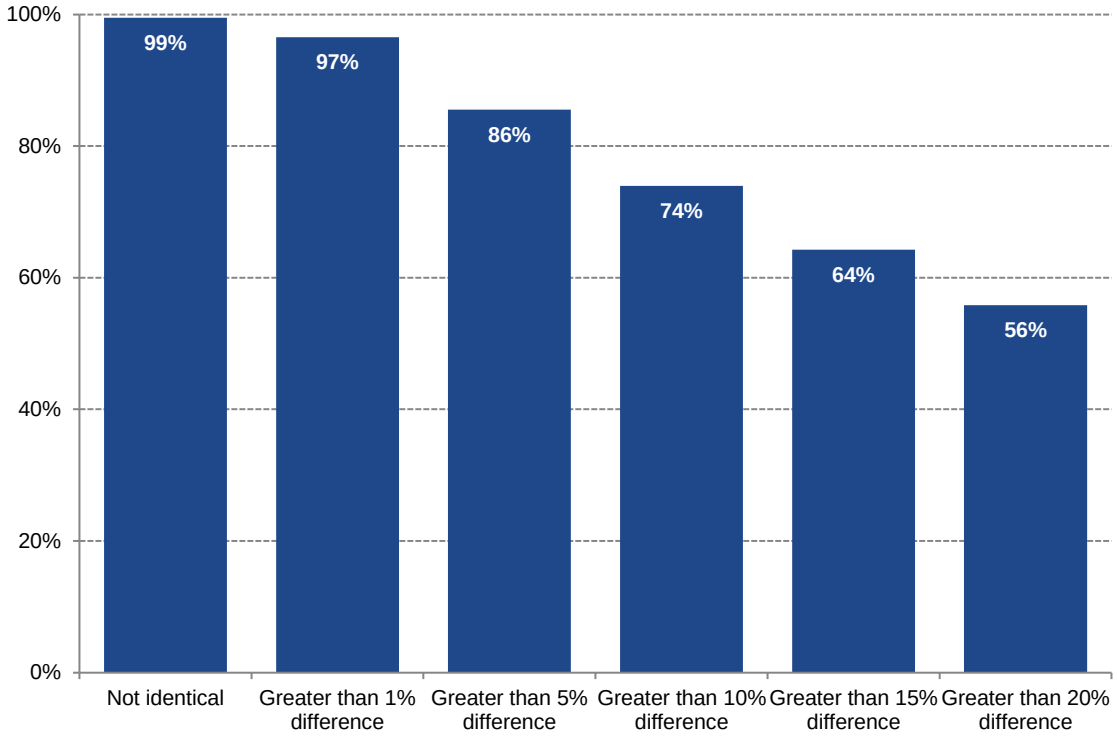
Sources: IPEDS; Federal Reserve Bank of St. Louis CPI data (as produced by Dr. Singer).
Notes:
[1] Average institutional grant aid calculated as the average amount of institutional grant aid awarded per full-time, first-time, degree-seeking undergraduate (total institutional grant aid awarded to full-time, first-time, degree-seeking undergraduates / total full-time, first-time degree-seeking undergraduates).
[2] The 2003–2004 academic year is the first academic year the consensus methodology guidelines could have potentially affected aid packages.

(82) A third way to quantify the amount of financial aid awarded by the Defendants is to measure the proportion of students awarded aid. Figure 9 depicts the percentage of full-time, first-time, degree-seeking undergraduates who received aid at the Defendants over time. It shows that while this percentage varies significantly from year to year, it has increased overall since the introduction of the

[107] The figure includes all the Defendants in all years, which includes periods when a Defendant was not part of the 568 Group. In Appendix C.1, I show the same chart limiting the chart to those six Defendants that were continuously part of the 568 Group (until its disbandment) since before the consensus methodology guidelines were introduced (Columbia, Cornell, Georgetown, MIT, Northwestern, Notre Dame). That chart looks very similar and also shows that average institutional aid, adjusted for inflation, awarded by those Defendants per undergraduate student has risen both over time and since the introduction of the consensus methodology guidelines.

Defendants agreeing to a common financial aid methodology or the challenged conduct standardizing expected family contributions.

Figure 14: Percentage of cross-admitted students who received different expected family contributions from the Defendants, 1998–1999 to 2022–2023 academic years

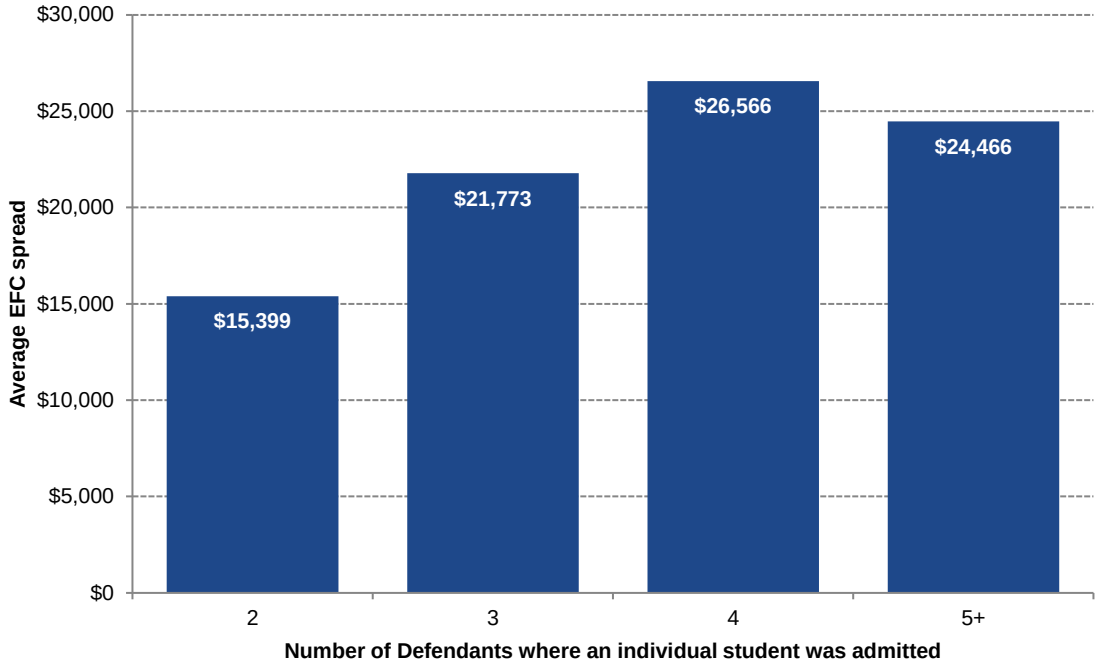


Sources: Corrected combined Defendant data; IPEDS; Cornell cost of attendance data; Federal Reserve Bank of St. Louis CPI data (as produced by Dr. Singer).
Notes:
[1] Data are limited to students who were admitted to two or more Defendants with non-missing and non-zero school-specific expected family contr butions.
[2] Data are restricted to the academic years during the conduct period for which each Defendant was a member of the 568 Group.
[3] Applicants for whom all expected family contributions exceed the cost of attendance are excluded from the analysis. I rely on IPEDS cost of attendance data to make this restriction, with the exception of Cornell. For Cornell, I rely on produced average cost of attendance data (CORNELL_LIT0000436889) that distinguishes between Cornell's endowed and contract colleges for New York residents.[133] *See* Section 3.3 for how cost of attendance is defined.

(102) Figure 15 depicts the average spread in expected family contribution for students who were admitted to two, three, four, and five or more Defendants. It shows that students who were admitted to two

[133] Some Cornell students pay lower tuition rates (*e.g.*, New York State residents matriculating at Cornell's contract colleges who pay in-state rates) that are not captured by IPEDS, which only reports the highest tuition rates (i.e., the tuition rates applicable to all students matriculating at Cornell's endowed colleges and students who are not New York State residents but matriculate at Cornell's New York State statutory colleges, or contract colleges). Before 2003, Cornell reported for purposes of IPEDS data separate metrics for its tuition (and thereby, cost of attendance) for its contract colleges. After 2003, a single metric is reported which overestimates the cost of attendance for New York State residents matriculating at Cornell's contract colleges, *see* my backup materials for a comparison of these metrics.

Figure 15: Average expected family contribution spread across the Defendants to which students were admitted from 2003–2022 (dollars in 2024 prices)



Sources: Corrected combined Defendant data; IPEDS; Cornell cost of attendance data; Federal Reserve Bank of St. Louis CPI data (as produced by Dr. Singer).
Notes:
[1] Expected family contr bution spread is calculated as the difference of the maximum school-specific expected family contribution minus the minimum school-specific expected family contribution for an individual applicant. The expected family contribution is deflated using the CPI to 2024 prices.
[2] Data are limited to first-year applicants who were admitted to two or more Defendants with non-missing expected family contributions.
[3] Data are limited to academic years in which a Defendant was a member of the 568 Group, beginning with the introduction of the consensus methodology guidelines, which would potentially affect students matriculating in the 2003–2004 academic year.
[4] Applicants for whom all expected family contributions exceed the cost of attendance are excluded from the analysis. I rely on IPEDS cost of attendance data to make this restriction, with the exception of Cornell. For Cornell, I rely on produced average cost of attendance data that distinguish between Cornell's endowed and contract colleges for New York residents. *See* Section 3.3 for how cost of attendance is defined.
[5] Within the Defendant data from 2003–2022, 33,683 students with non-missing school-specific expected family contributions were admitted to two Defendants, 7,875 students were admitted to three Defendants, 1,996 students were admitted to four Defendants, and 656 students were admitted to five or more Defendants.

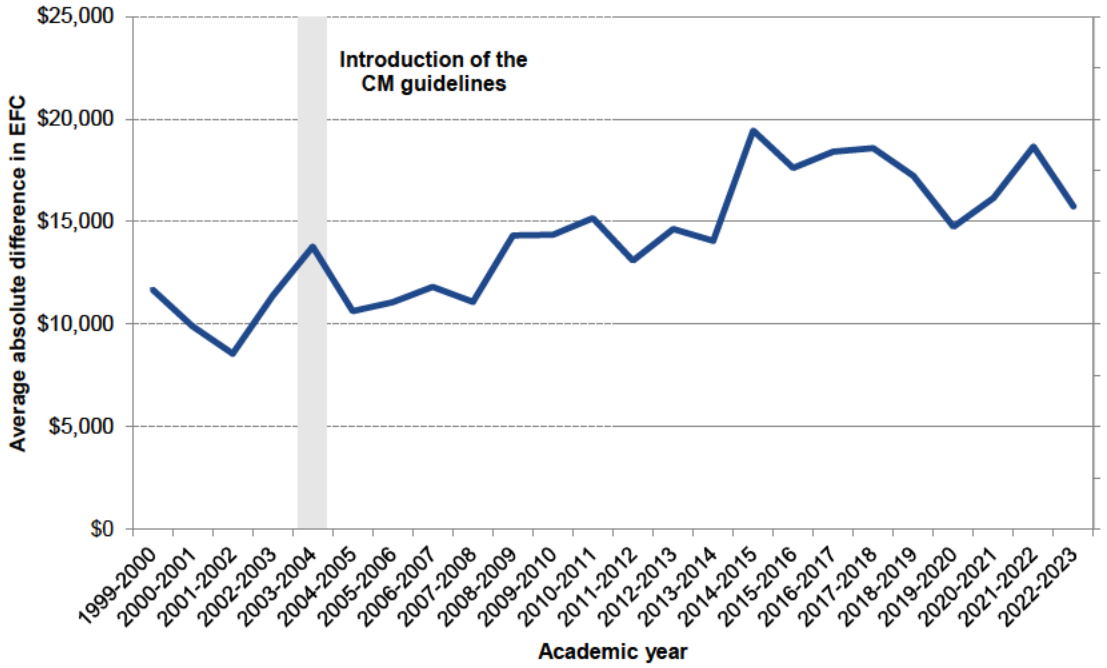
5.1.2. The introduction of the consensus methodology guidelines did not change the variation in the Defendants' expected family contribution calculations

(103) As discussed in Section 3.2.2, the 568 Group's consensus methodology guidelines could have first affected students matriculating in the 2003–2004 academic year.[135] If the Defendants standardized

[135] *See* Section 3.2.2.

2002 introduction of the consensus methodology guidelines, which would affect students matriculating in the 2003–2004 academic year.

Figure 17: Average variation in school-specific expected family contribution estimates across the Defendants (dollars in 2024 prices)

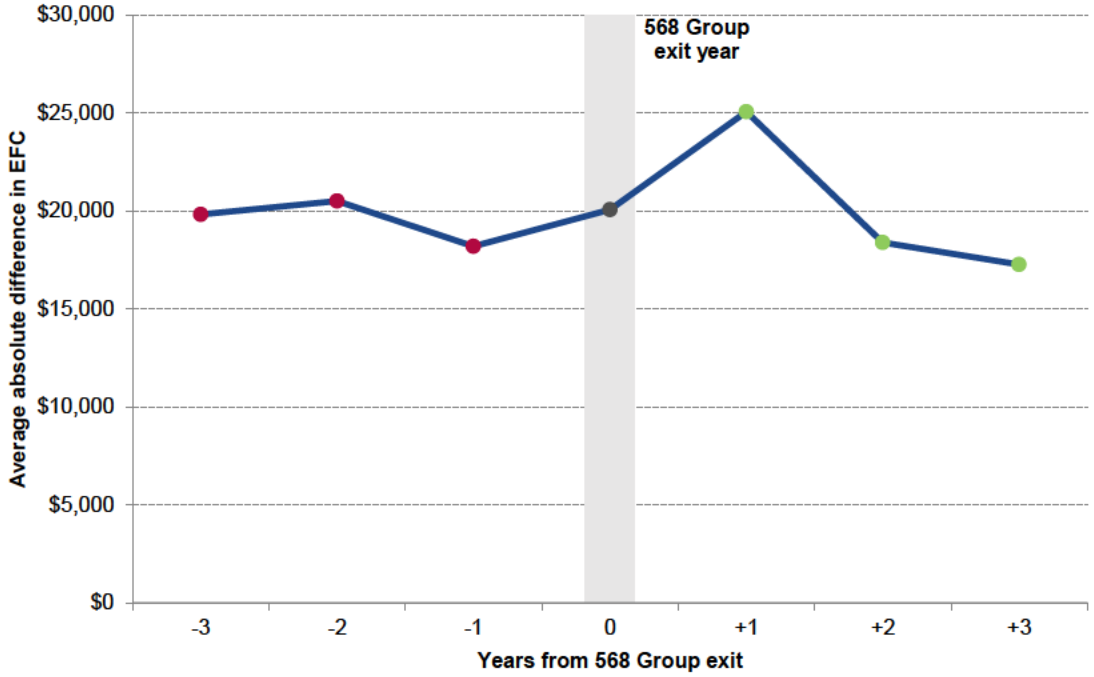


Sources: Corrected combined Defendant data; IPEDS; Cornell cost of attendance data; Federal Reserve Bank of St. Louis CPI data (as produced by Dr. Singer).
Notes:
[1] The expected family contribution is deflated using the CPI to 2024 prices. Data are limited to first-year applicants who were admitted to two or more Defendants with non-missing expected family contributions.
[2] Applicants for whom all expected family contributions exceed the cost of attendance are excluded from the analysis. I rely on IPEDS cost of attendance data to make this restriction, with the exception of Cornell. For Cornell, I rely on produced average cost of attendance data that distinguish between Cornell's endowed and contract colleges for New York residents. *See* Section 3.3 for how cost of attendance is defined.
[3] Availability of Defendant data varies from the 1999–2000 to 2022–2023 academic years. Additionally, some Defendants enter and exit the 568 Group at different times throughout the period.
[4] The 2003–2004 academic year is the first academic year the consensus methodology guidelines could have potentially affected aid packages.

(107) The figure shows that variation in real expected family contributions in 2024 dollars across the Defendants did not materially change after 2003. Indeed, if anything, the variation increased. These findings further corroborate the analyses previously performed in this section.

(108) The evidence in this section is inconsistent with Plaintiffs' theory and Dr. Singer's assertion that the consensus methodology guidelines standardized expected family contributions across the Defendants. My findings are consistent instead with the introduction of the 568 Group's consensus methodology

Figure 18: Average variation in school-specific expected family contribution estimates, 568 Group exit (dollars in 2024 prices)



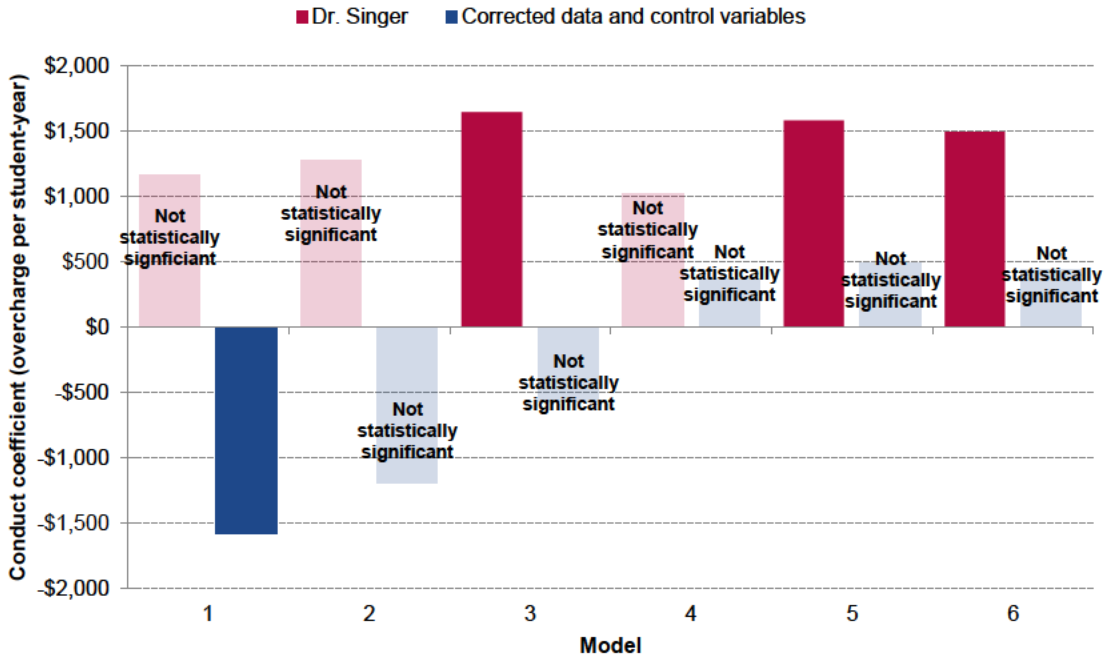
Sources: Corrected combined Defendant data; IPEDS; Cornell cost of attendance data; Federal Reserve Bank of St. Louis CPI data (as produced by Dr. Singer).
Notes:
[1] This figure is limited to the six Defendants that exited the 568 Group prior to the group dissolving in 2022 and produced three years of post-exit data (Brown, Emory, Penn, Rice's first exit, U-Chicago, and Yale's first exit). Vanderbilt's exit is excluded, as its 2023–2024 data are incomplete, resulting in less than three years of post-exit data to analyze. Rice's second exit and Duke's exit are excluded, as there are not three years of post-exit data to analyze. *See* Section 3.2.3 for a full list of Defendant exit dates.
[2] For each entrant, I limit the set of Defendants for which I compare expected family contributions to those for whom data are available for the entire seven-year period being analyzed.
[3] The expected family contribution is deflated using the CPI to 2024 prices.
[4] Data are limited to first-year applicants who were admitted to two or more Defendants with non-missing expected family contribution calculations.
[5] Applicants for whom all expected family contributions exceed cost of attendance are excluded from the analysis. I rely on IPEDS cost of attendance data to make this restriction, with the exception of Cornell. For Cornell, I rely on produced average cost of attendance data that distinguish between Cornell's endowed and contract colleges for New York residents. *See* Section 3.3 for how cost of attendance is defined.

5.1.4. Qualitative evidence is consistent with the Defendants calculating expected family contributions differently

(112) To further corroborate my analyses above, I reviewed qualitative evidence on this topic. The qualitative evidence is consistent with my analyses showing that the Defendants varied in their calculation of expected family contributions.

show that Dr. Singer's regressions do not support his conclusion of overcharges once his data and control variables are corrected.

Figure 33: Estimated impact of alleged conduct on effective institutional price using Dr. Singer's data and corrected data and control variables, based on Dr. Singer's model

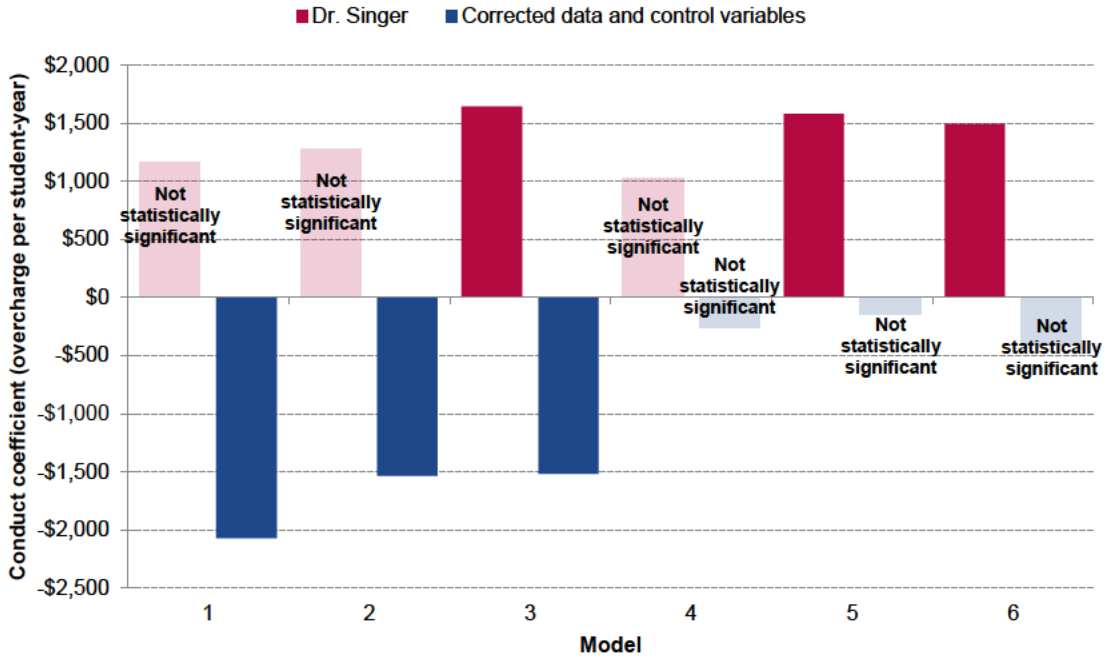


Source: Corrected combined Defendant data.
Notes:
[1] Bars represent the conduct estimates from Dr. Singer's Table 11 original models and data (red bars) and using the corrected data, including updates to the dependent variable, class members, and controls (blue bars).
[2] Effective institutional price is regressed on school fixed effects and various control variables in models 1–3, and student-school fixed effects and various control variables in models 4–6.
[3] "Not statistically significant" means the results are not significant at the 5 percent level using two-way clustering.

8.3. Dr. Singer's overcharge model finds that the challenged conduct's effect was neutral when controlling for rising costs and macroeconomic conditions affecting higher education

(206) In this section, I address the fact that Dr. Singer fails to adequately control for changing conditions that may affect supply and demand conditions for higher education. For example, his model does not adequately control for the changing cost of higher education and the lasting impact of shocks such as the COVID-19 pandemic and subsequent, rapid inflationary pressures. These factors may be unrelated to the challenged conduct, but if they are not adequately controlled for, they might be captured by Dr. Singer's estimated impact of the challenged conduct, rendering his results unreliable.

Figure 39: Dr. Singer's model with corrected data and post-COVID-19 controls



Source: Corrected combined Defendant data.
Notes:
[1] Bars represent the conduct estimates from Dr. Singer's Table 11 (red bars) and correcting data for Dr. Singer's model and using dummies for each year post-COVID-19 (blue bars).
[2] Effective institutional price is regressed on school fixed effects and various control variables in models 1–3, and student-school fixed effects and various control variables in models 4–6.
[3] "Not statistically significant" means the results are not significant at the 5 percent level using two-way clustering.

8.3.3. Dr. Singer's opinions depend upon including flawed post-conduct data

(224) In this section, I show that Dr. Singer's conclusions depend upon including data from two years after the alleged conduct, academic years 2023–2024 and 2024–2025. This dependency upon data from these years is problematic because there are several reasons data from these years should be excluded.

(225) First, the 2023–2024 and 2024–2025 academic year data have data limitations that make estimates based on those years unreliable. For example:

- The 2024–2025 academic year data are incomplete: only four Defendants (Cornell, Georgetown, Johns Hopkins, and Penn) produced data reporting any institutional financial aid awards in that year. What is more, the data produced by these Defendants are incomplete and contain only preliminary estimates of students' financial aid awards. Georgetown, for example, stated in a letter accompanying its data production that it "only includes early-decision award data for academic year 2024–2025, to the extent available" and that "because the aid process is still

ongoing for the 2024–2025 academic year, this data is necessarily tentative and incomplete."[355] Cornell similarly stated in a letter accompanying its data production that the data for academic year 2024–2025 reflect "institutional aid award estimates."[356]

- The 2023–2024 academic year data are incomplete: the number of students and total institutional awards reported in the data for many Defendants drop off substantially in academic year 2023–2024 relative to earlier years. For example:
  - [redacted][357]
  - Only 135 students appear as having received institutional aid in the Caltech data in 2023, compared to nearly 500 students receiving institutional aid in 2022.
  - For six Defendants that produced data containing information on paid awards, the paid award fields are not populated in 2023–2024.
- Dr. Singer's data on his institutional control variables—i.e., (1) institutional tuition revenue per full-time equivalent undergraduate (one-year lag) and (2) percent of full year-full-time equivalent undergraduates receiving financial aid—come from the Integrated Postsecondary Education Data System (IPEDS) and other public sources and are only available until 2022–2023. Hence, Dr. Singer has to extrapolate data for his institutional control variables for the academic years 2023–2024 and 2024–2025.[358]

(226) Second, while Dr. Singer includes data for the 2023–2024 and 2024–2025 academic years to estimate his model, he claims that the interpretation of these data is uncertain because the impact of the challenged conduct could have persisted beyond the end of the 568 Group.[359] He further asserts that including these data therefore makes his findings conservative because he assumes in using them that there was no lingering impact.[360] These claims are unavailing: in fact, far from being conservative,

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355 Letter from Daniel Fenske (Mayer Brown) to Edward Normand (Freedman Normand Friedland), Robert Gilbert (Gilbert Litigators & Counselors), and Robert Litan (Berger Montague), May 17, 2024.

356 Letter from Emily Chen (Kirkland & Ellis) to Robert Litan (Berger Montague), Edward Normand (Freedman Normand Friedland), and Robert Gilbert (Gilbert Litigators & Counselors), March 29, 2024.

357 Duke admissions and financial aid data, *see* Appendix J.

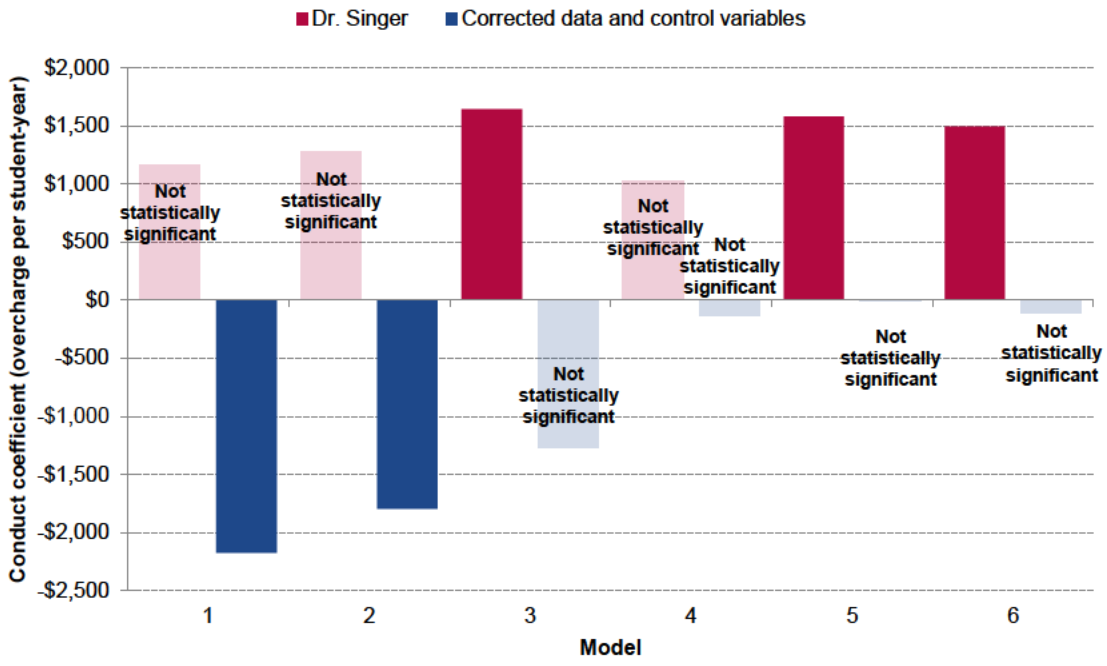
358 Singer Report, n. 328 ("The institutional level control variables from IPEDS are reported for 1998-2021, and the endowment data goes from 1999-2021. I supplement missing 2022 institutional tuition revenue data and missing 2022-2023 undergraduate enrollment data using public sources (*e.g.*, Defendants' websites). For remaining missing observations, I extrapolate the institutional control variables for each Defendant by regressing the control variable on a linear time trend and a COVID dummy variable, and then using the predicted value from this regression to replace the missing value.").

359 Singer Report, ¶ 228 ("[M]y method credits the claims by Defendants to have withdrawn from the 568 Group and counts these periods as "clean." But economics suggests that the prices set by participating in the cartel, as the Challenged Conduct is alleged to have been, do not necessarily fall back to competitive levels immediately. Thus, for these reasons, my benchmark includes prices that may well be inflated by the Challenged Conduct, and thus would tend to understate the Challenge Conduct's true economic effects on prices.").

360 Singer Report, ¶ 228.

these data are required for Dr. Singer to find any statistically significant evidence of the challenged conduct having an effect. Figure 40 displays the overcharge regressions' estimates with (in red) and without (blue) data for academic years 2023–2024 and 2024–2025. The figure shows that if one excludes these two years, Dr. Singer's models find no statistically significant evidence to support an adverse impact from the challenged conduct (and indeed they suggest that the challenged conduct may have been beneficial to students). The evidence in the figure establishes that Dr. Singer's claim that including these years is conservative is false. Instead, Dr. Singer relies upon these years to find any statistically significant evidence of an adverse effect.[361]

Figure 40: Dr. Singer's model when using corrected data and dropping 2023–2024 and 2024–2025



Source: Corrected combined Defendant data.
Notes:
[1] Bars represent the conduct estimates from Dr. Singer's Table 11 (red bars) and correcting data for Dr. Singer's model dropping 2023–2024 and 2024–2025 (blue bars).
[2] Effective institutional price is regressed on school fixed effects and various control variables in models 1–3, and student-school fixed effects and various control variables in models 4–6.
[3] "Not statistically significant" means the results are not significant at the 5 percent level using two-way clustering.

(227) In summary, Dr. Singer's purported findings of adverse effects depends heavily upon including data from academic years 2023–2024 and 2024–2025. These data are flawed and should not be included

[361] Similarly, Dr. Singer's log-linear model estimates, those including merit-based aid (in levels and logs), and with net price as the dependent variable (in levels and logs) are not robust to excluding academic years 2023–2024 and 2024–2025 with none of the models showing statistically significant overcharges and models 1–3 implying statistically significant decreases in price due to the alleged conduct; *see* Appendix H.4.

damages) holds for 29 percent of school-academic year combinations during the alleged conduct period.[366]

(233) These problems arise due to an econometric issue with Dr. Singer's model. The problem results from the combination of several factors, including the facts that (1) Dr. Singer's models 4–6 use a single dummy variable to identify the effect of the challenged conduct and (2) schools enter and exit the challenged conduct at different times.[367] This results in affected students serving as a control for other affected students.[368]

8.5. Dr. Singer has failed to account for selection bias introduced by the fact that the alleged conduct is associated with a changing fraction of students on full rides

(234) Dr. Singer's overcharge modeling is plagued by multiple forms of bias in the selection of his sample. Specifically, before estimating his regression model Dr. Singer limits his sample to students (1) whose grant aid covered less than 95 percent of the school-academic year specific cost of attendance and (2) who received a positive amount of institutional need-based grant aid.[369] Both sources of selection bias can result in an estimate of positive overcharges due to the alleged conduct when every individual class member was either unharmed or benefitted from the actions of 568 Group.[370]

(235) To illustrate this issue, I present two examples. The first demonstrates the issue with Dr. Singer excluding students whose grant aid covered more than 95 percent of the cost of attendance. The second demonstrates the issue with Dr. Singer excluding students who do not receive any form of grant aid. I will abstract from Dr. Singer's exact overcharge models for the sake of simplicity and consider the general form of Dr. Singer's "before-after" modeling.

(236) My first example is illustrated in Figure 42. Suppose there are two students, Student A and Student B, both of whom attend the same School S, for two years. School S is not a member of the 568 Group in the first year but joins prior to the second year. Now suppose that Student A has an effective

366 *See* my backup materials. I calculate analogous percentages for all regressions in Dr. Singer's Table 11 regressions.

367 In my analysis in Section 4.2, I mitigate the econometric difficulties highlighted in this section. I do so by using a methodology designed to overcome them.

368 Recall that Dr. Singer's models 4–6 identify the effect of the challenged conduct by comparing the financial aid granted to students whose schools changed 568 Group membership status to the financial aid granted to students whose schools did not change 568 Group membership status. This control group includes both students whose schools were in (and remained in) the 568 Group and students whose schools were not in (and remained out of) the 568 Group. It also compares students in different time periods to one another. These comparisons lead to counter-intuitive findings such as the ones described above.

369 Singer Report, ¶ 246.

370 *See, e.g.*, Joshua D. Angrist and Jörn-Steffen Pischke, *Mostly Harmless Econometrics: An Empiricist's Companion* (Princeton University Press, 2009), § 2.1.

institutional price of $30,000 in both years. Student B has an effective institutional price of $10,000 for the first year, and $0 for the second (i.e., student B receives a full ride the second year). Dr. Singer's "before-after" modeling would calculate an average effective institutional price of $20,000 for the "before" period (the average of Student A's $30,000 effective institutional price and Student B's $10,000 effective institutional price in the first year). Dr. Singer's approach would calculate an average effective institutional price of $30,000 for the conduct period (Student A's effective institutional price is $30,000 and Student B's price is excluded from the Dr. Singer's "before-after" as Student B received a full ride). As a result, Dr. Singer's approach would estimate an *increase* in the effective institutional price by $10,000 ($20,000 during the "before" period vs. $30,000 during the conduct period) due to the alleged conduct when neither Student A nor Student B's effective institutional price increased due to the alleged conduct—indeed, Student B's price decreased. This is an obvious modeling error, since Student A pays the same amount each year, while Student B pays less the second year.

Figure 42: Example illustrating sample selection issues from full ride students in Dr. Singer's overcharge modeling

	Year 1 price (pre-conduct period)	In Dr. Singer's calculation?	Year 2 price (conduct period)	In Dr. Singer's calculation?	Difference
Student A	$30,000	Yes	$30,000	Yes	$0
Student B	$10,000	Yes	$0	No	-$10,000
True average	$20,000		$15,000		**-$5,000**
Dr. Singer's average	$20,000		$30,000		**$10,000**

Note: Details of this example can be found in the text above.

(237) My second example is illustrated in Figure 43. Suppose there are two other students (Student C and Student D) who attended the same School S in the same two-year period as my previous example. In this example, the effective institutional price faced by Student C is $10,000 for both the first and second year (the "before" year and the conduct year). Suppose further that Student D received no aid in the first year and therefore paid the full cost of attendance of $50,000. Additionally, Student D received some aid in the second year and had an effective institutional price of $30,000. Dr. Singer's before-after model would compute an average effective institutional price of $10,000 in the first year (Student D's first year price is dropped from the analysis, as they did not receive any grant aid and the only price used in the first year is Student C's effective institutional price). Dr. Singer's model would compute an average effective institutional price of $20,000 in the conduct year (the average of Student C and Student D's prices of $10,000 and $30,000 in the second year, respectively). Dr. Singer's before-after approach would find a $10,000 increase in the effective institutional price for class members due to the conduct—the average effective institutional price increased from $10,000 in the "before" period to $20,000 in the conduct period. However, neither Student C nor Student D's effective institutional price increased from before the conduct to the conduct period, and Dr. Singer would erroneously conclude that the alleged conduct increased the effective institutional price of class members.

Figure 43: Example illustrating sample selection issues from students receiving any aid in Dr. Singer's overcharge modeling

	Year 1 price (pre-conduct period)	In Dr. Singer's calculation?	Year 2 price (conduct period)	In Dr. Singer's calculation?	Difference
Student C	$10,000	Yes	$10,000	Yes	$0
Student D	$50,000	No	$30,000	Yes	-$20,000
True average	$30,000		$20,000		**-$10,000**
Dr. Singer's average	$10,000		$20,000		**$10,000**

Note: Details of this example can be found in the text above.

(238) The previous two examples illustrate the selection issues in Dr. Singer's general "before-after" model that prevent one from interpreting his model estimates as reflective of the effect of the alleged conduct on the prices class members paid to the Defendants.

8.6. Damages

(239) After correcting for errors in Dr. Singer's model and corresponding damages calculations, there is no credible evidence that the class members were harmed by the alleged conduct. Hence, I conclude that Dr. Singer's report provides no credible evidence of anticompetitive harm as a result of the alleged conduct.

Figure 94: Significance levels for the difference in correlation between net price and lagged excess returns during the pre-conduct and conduct periods, with and without corrected standard errors

Regression	Difference in lagged excess returns coefficient	Standard error	p-value	Significance level		
				1%	5%	10%
Dr. Bulman's uncorrected standard errors	0.259	0.121	0.034	No	Yes	Yes
Corrected standard errors	0.259	0.142	0.086	No	No	Yes

Source: Dr. Bulman's Defendants FA Regression Data from Dr. Bulman's backup materials.
Notes:
[1] This table shows the estimated difference in lagged excess returns coefficient for the pre-conduct period and conduct period, and the associated standard errors and statistical significance levels.
[2] The model I use is identical to the model that Dr. Bulman uses for Table 7 and Table 8 in his report, except that I incorporate the pre-conduct and conduct periods into a single regression to be able to conduct an appropriate hypothesis test.
[3] Uncorrected standard errors refers to Huber-White robust standard errors. Corrected standard errors refers to standard errors clustered at the school level.

Figure 95: Significance levels for the difference in correlation between institutional aid and lagged excess returns during the pre-conduct and conduct periods, with and without corrected standard errors

Regression	Difference in lagged excess returns coefficient	Standard error	p-value	Significance level		
				1%	5%	10%
Dr. Bulman's uncorrected standard errors	-0.259	0.082	0.002	Yes	Yes	Yes
Corrected standard errors	-0.259	0.126	0.056	No	No	Yes

Source: Dr. Bulman's Defendants FA Regression Data from Dr. Bulman's backup materials.
Notes:
[1] This table shows the estimated difference in lagged excess returns coefficient for the pre-conduct period and conduct period, and the associated standard errors and statistical significance levels.
[2] The model I use is identical to the model that Dr. Bulman uses for Table 7 and Table 8 in his report, except that I incorporate the pre-conduct and conduct periods into a single regression to be able to conduct an appropriate hypothesis test.
[3] Uncorrected standard errors refers to Huber-White robust standard errors. Corrected standard errors refers to standard errors clustered at the school level.

I.2. Dr. Bulman's methodology in his academic work

(300) In Section 9.2, I show that Dr. Bulman's assumption that the Defendants' lagged excess returns and their endowments are correlated is incorrect for the challenged conduct period. Hence, his regression of financial aid spending on lagged excess returns does not support his argument that when the Defendants' endowments increased in the conduct period, they did not spend more on financial aid. In this section I replicate the methodology of Dr. Bulman's 2022 NBER working paper to test whether the effects of endowments on institutional grant aid, net price, and effective institutional price are significantly different between the pre-conduct and conduct periods as Dr. Bulman claims in his

report.[426, 427] The results based on Dr. Bulman's working paper methodology contradict those in his report and indicate that the effects of endowments on financial aid spending and prices were not significantly different between the pre-conduct and conduct periods.

(301) Dr. Bulman's working paper methodology involves two steps. The first step is to construct an instrumental variable to identify the causal effect of endowments on financial aid spending. Following Dr. Bulman's approach,[428] I construct a "simulated endowments" variable that is only correlated with investment returns. Specifically, "annual returns are applied to the baseline endowment and then this value is reduced by a fixed percent to reflect average annual spending out of the endowment."[429] Under Dr. Bulman's assumption in his report that the Defendants have "the same effective spending rate of 4.7%," the simulated endowments variable is equivalent to the lagged excess returns Dr. Bulman uses in his report.[430] According to Dr. Bulman, this simulated endowments variable can be used as an instrument to "isolate the causal effect of endowments on spending [and] the generosity of financial aid."[431]

(302) The second step of Dr. Bulman's working paper methodology is to estimate an instrumental variable regression using simulated endowments as an instrument.[432] To evaluate whether the effect of endowments on financial aid spending is significantly different between the pre-conduct and conduct periods, I implement the instrumental variable estimation using two approaches: (1) a pooled sample for the entire sample period combining the pre-conduct and conduct periods,[433] and (2) Dr. Bulman's two separate samples: one for the pre-conduct period and the other for the conduct period.

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426 George Bulman, "The Effect of College and University Endowments on Financial Aid, Admissions, and Student Composition," NBER Working Paper, no. 30404 (August 2022).

427 Bulman Report, ¶ 45.

428 George Bulman, "The Effect of College and University Endowments on Financial Aid, Admissions, and Student Composition," NBER Working Paper, no. 30404 (August 2022).

429 George Bulman, "The Effect of College and University Endowments on Financial Aid, Admissions, and Student Composition," NBER Working Paper, no. 30404 (August 2022), p 14.

430 Since the regression model includes school fixed effects and endowments are expressed in logs, the value of baseline endowments does not affect the regression results. Therefore, assuming the baseline endowment is one, simulated endowments are equivalent to lagged excess returns that Dr. Bulman uses in his report. Note that I use the measure of lagged excess returns from Dr. Bulman's report, which is deflated by HEPI. Bulman Report, ¶ 35.

431 George Bulman, "The Effect of College and University Endowments on Financial Aid, Admissions, and Student Composition," NBER Working Paper, no. 30404 (August 2022), pp. 9 and 14–15.

432 In general, instrumental variables can identify the causal effect of independent variables on the outcome variable as long as the instrumental variables are (1) uncorrelated with other uncontrolled factors that affect the outcome variable and (2) correlated with the independent variables. *See, e.g.,* Jeffrey M. Wooldridge, Introductory Econometrics: A Modern Approach, 6th ed. (South-Western Cengage Learning, 2013), pp. 461–465, 471–477. George Bulman, "The Effect of College and University Endowments on Financial Aid, Admissions, and Student Composition," NBER Working Paper, no. 30404 (August 2022).

433 The model regresses each of the outcome variables on pre-conduct period endowments (endowments interacted with a pre-conduct period dummy) and conduct-period endowments (endowments interacted with a conduct period dummy) with school fixed effects and year fixed effects, where the endowment variables are instrumented by the simulated endowments in the pre-conduct period (simulated endowments interacted with a pre-conduct period dummy) and the simulated endowments in the conduct period (simulated endowments interacted with a conduct period dummy).